LETTERS TO THE EDITOR

Antibiotic treatment of suspected neonatal meningitis

Sir,—The annotation on antibiotic treatment of suspected neonatal meningitis seems to demonstrate that optimal treatment of this thankfully rare disorder is still not clear.1 The suggested array of antibiotic treatment and combined routes of administration may provide cover against a wide range of organisms, but is such treatment in the best interests of every patient? In a sick hypotensive infant who may already have compromised renal function the combination of aminoglycoside and cephalosporin may exacerbate the problem. To state that 'Gentamicin should be used primarily to treat the associated septicaemia' is in the implication that cephalosporins are incapable of doing this. Is there any evidence to suggest that cefotaxime is ineffective in the treatment of bacteremia in neonates?

I am concerned by the conviction of the authors in their management proposals. To begin a paragraph stating 'there is no doubt in our minds that intraventricular treatment should be used' and end it with 'there has been no study of intraventricular treatment . . . and little information on the ventricular drug concentration achieved after systemic treatment alone in dilated ventricles' is perhaps a little rash in view of the finding of significantly increased mortality after intraventricular treatment in the American study.2

The conclusion of McCracken and colleagues was that 'intraventricular therapy cannot be recommended for the routine management of neonatal meningitis caused by Gram negative enteric bacilli',3 in the absence of a further randomised multicentre trial it is surely ill advised to recommend the adoption of this invasive and potentially dangerous mode of treatment as the standard treatment in neonatal Gram negative meningitis with ventricular dilatation.


Dr Rennie and Gandy comment:

Thank you for giving us the opportunity to reply to the correspondence regarding the editorial. Opinions regarding the benefits of intraventricular treatment in neonatal Gram negative meningitis are likely to continue to differ for some time: our recommendations constitute a step towards consensus and to provide guidelines for the management of this serious and rare disease. We remain of the opinion, shared with Pearse and Robertson,4 that a ventricular tap should be performed if the ventricles are large or the infant is failing to respond to treatment after 24 hours. Intraventricular treatment should be given via a Rickham reservoir to babies with ventriculitis. We do not avoid this treatment because of the conclusions of the Neonatal Meningitis Cooperative Study Group because patients were enrolled in this study more than 10 years ago, before the widespread availability of cranial ultrason or the third generation cephalosporins. Much of the discussion pertaining to this work has been well rehearsed before, including the fact that only 20 cases were actually neonates suffering meningitis.

The combination of cefotaxime with another drug such as gentamicin was suggested because of the evidence in favour of synergism, the improved outlook in gram-negative meningitis with intravenous gentamicin combined with intraventricular therapy, and the emergence of resistance. The advice of our microbiologists has always been to start treatment with a dual regime to obtain rapid bacterial killing. We use a single agent when the baby is improving and sensitivity results are available. Ceftriaxone monotherapy seems promising in adults and may prove useful in the neonate. The precise nature of the appropriate dual therapy is precisely the type of complication that led us to suggest transfer of such cases to centres with intensive care facilities.

Dr Tarchow's suggestion that neonatologists should consider high dose dexamethasone is an interesting one and we are aware of the evidence suggesting a reduction in neurological sequelae in older children, most of whom were suffering from haemorrhagic infection.4 The presence of dexamethasone in neonatal meningitis is planned.3 The dexamethasone has been well rehearsed before, including the fact that only 20 cases were actually neonates suffering meningitis. We do not avoid this treatment because of the conclusions of the Neonatal Meningitis Cooperative Study Group because patients were enrolled in this study more than 10 years ago, before the widespread availability of cranial ultrason or the third generation cephalosporins. Much of the discussion pertaining to this work has been well rehearsed before, including the fact that only 20 cases were actually neonates suffering meningitis.

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When should the coeliac patient have an intestinal biopsy?

Sir,—Gluten challenges and intestinal biopsies are stressful procedures both for coeliac patients and for the doctors dealing with them. While we recognise that the time has
come to reconsider the 1970 European Society of Paediatric Gastroenterology and Nutrition (ESPGAN) diagnostic criteria for gluten sensitiv-
ity enteropathy,1 we do not entirely agree with the timing of the diagnostic intestinal biopsy proposed by our Italian Group of Paediatric Gastroenterology at the meeting in Trieste, May 1987, and recently published.2 A multicentre study indicates that in Italy the vast majority of diagnoses of gluten sensitive enteropathy (more than 90%) are made in young children who are referred with signs of malabsorption or diarrhea and are then found positive for the antigliadin antibody test. At the Trieste conference it was stated that these so called typical cases do not need a gluten challenge and can be readily and definitely diagnosed by a single biopsy specimen showing a flat mucosa at the first work up.

Looking back on our hundreds of biopsies we have noticed that whenever the clinical and laboratory data strongly suggested gluten sen-
sitive enteropathy, we nearly always found a flat mucosa at the first intestinal biopsy. It appears then that the initial biopsy recom-
ended by ESPGAN and retained by the Trieste recommendations is seldom informa-
tive as its result is highly predictable in typical patients. In these cases (supported by a posi-
tive antigliadin antibody test and abnormal intestinal permeability test) we suggest that the initial biopsy is avoided and a six to 12 month period of gluten free diet is commenced (figure).

As the first years of a gluten free diet seem to have an imprinting effect on the long term compliance, we believe that the gluten chal-
lenge is, at present, unavoidable in the youngest patients because it demonstrates the persistence of, the degree to, the patient’s family. At the end of a three to six month period of gluten challenge (or less if symptoms develop) a biopsy is highly recommended as the relapse is sometimes evident only at the histological level. This single biopsy specimen will confirm the diagnosis of gluten sensitive enteropathy in most cases. If the mucosa looks normal the patient can be left on a free diet and periodically checked to exclude the possibil-
ity of a late response to gluten. In our opin-
ion a short period of gluten ingestion, as we propose, should not expose the patient to significant risks.

The above approach is less invasive than the ESPGAN scheme and fully exploits the diagnostic role of the antigliadin antibody test at the first assessment of the patient. We also suggest adding the sugar intestinal permeabili-
ty test, which has proved to be a reliable screen-
ing procedure for gluten sensitive enteropathy.2 For atypical or late onset cases we agree that an intestinal biopsy is always mandatory during the first diagnostic phase. A conclusive note of caution is needed. It is well known that long term treatment of gluten sensitive enteropathy is often unsatisfactory.3 This is especially true for patients who have found ‘positive’ doctors who are overzealous in their wish to avoid unpleasant biopsies. So far we have followed the ESPGAN diagnostic criteria for coeliac disease with satisfactory results both for patients and for us. Neverthe-
less, in the light of the new, non-invasive diagnostic tools, we also agree on the need of revising the ESPGAN recommendations. This attempt should be made with the contribution of experts from several European centres.

C CATASSI
G NATALINI
M OSSINI
M RATSCH
G VPPA
P L GIORGI
Department of Paediatrics, University of Ancona, Via Corridoni 11, 60123 Ancona, Italy


Urinary growth hormone excretion

Sir,—The paper by Walker and colleagues on the use of urinary growth hormone excretion as a screening test for growth hormone defi-
ciency, Arch Dis Child 1990; 65:811–9,2 may be negative. At all it could at least for the subjects who were not growth hormone deficient, if any correlation exists at all it could possibly be negative. There are clearly several factors that may influence urinary growth hormone excretion, and more detailed statistical analysis of the separate groups might have been more infor-
mative. The correlation coefficients would of course have been less impressive.

Walker J, Wood PJ, Williamson S, Betts PR, Evans AJ. Urinary growth hormone excretion as a screening test for growth hormone defi-
2 Pulse 1990 January 20:3.

Dr Walker comments:

The term has raised questions on certain aspects of our paper that I would like to comment on.

The purpose of presenting the relationship between overnight urinary growth hormone and peak serum response to conventional pro-
 vocative tests (fig 4) was purely to demonstrate that this simple urine test is as good as the cur-
rently available blood tests in identifying those children whose growth problems are due to a rela
tive deficiency of growth hormone. Taking all those studied prospectively the relationship between the log transformed urinary growth hormone concentrations and peak serum response to provocation holds (r=0.65, p<0.001) and according to advice from our statisticians is quite valid. To undertake ‘a more detailed statistical analysis’ would attach far too great a significance to this relationship especially with the controversy surrounding the interpretation of provocation tests.

The data that best evaluates the role of urin-
ary growth hormone excretion as a screening test for growth hormone deficiency is that of the children from the short stature group in figure 3. There were all studied prospectively with concurrent urine and blood tests and the urin-
ary growth hormone results compared with the reference range for a normal population (fig 3). In fact we have since studied at least another 100 similar children and have not yet missed a child with growth hormone defi-
ciency on the basis of low serum growth hormone concentrations in response to pro-
vocation but a normal urinary growth hormone. Such is our experience and confi-
dence in the method that we now use it as a first line screen for growth hormone defi-
ciency and offer it as part of our regional endocrine service to Wessex paediatricians.